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Reply to Valverde

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L. James Valverde A., Jr.[1] in discussing arguments put forward
in my 1990 paper, provides many good reasons for rejecting a
philosophy of risk which attempts to ground the concept ontologically,
i.e., on the basis of some object or class of objects that exist
independently of human cognition. However, he misrepresents the
central aim of my paper, in asserting that I wished to defend such a
view. In fact, I rejected the view he criticized and advocated one quite
like his claim that "... risk is not independent of human knowledge, and
is *fundamentally an epistemic category....*"[2] in the 1986 paper he
cites.[3] Valverde and I do have a difference of opinion, however, with
respect to the centrality of probability theory in characterizing what he
aptly calls the cognitive status of risk. In this reply, I will clarify my
position. In doing so, I will also present reasons for rejecting
Valverde's approach to the cognitive status of risk.

Nicholas Rescher[4] presented the most coherent statement of the
view that risk could be understood as a real object in the book Valverde
cites, but there are many hints at an ontological account of risk in the

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literature.[5] The ontological view is incoherent for the reasons Valverde discusses in his paper, as well as for reasons I have discussed in a series of papers and reviews.[6] Aside from Rescher's careful statement of the ontological view, however, no author successfully distinguishes an explicit philosophical commitment to the ontological view from a common-sensical commitment to basing risk policies on what is known about the technology, substance, or practice in question, rather than upon speculative scenarios. On this more casual use of language, "real," and "perceived," do not name ontological categories,

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[5]   The view that Valverde attributes to me is probably most expressly stated in the 1976 Starr, Rudman and Whipple paper that he cites, but the suggestion that there are natural facts of risk which are misperceived by the general public has become dogma among scientists who study hazards. For example, H.W. Lewis [in his TECHNOLOGICAL RISK at 33–31 (1990)] describes the well-known phenomenon of familiarity as follows:

Ghost story writers, carnivals and demagogues exploit fear of the unknown. This is one reason why low-probability risks often seem worse than those with high-probability — they are bound to be less familiar. ... People exaggerate the risk in the unaccustomed. ... Despite the essentially unanimous view of informed scientists that the risk of [nuclear power] is grossly overrated, the fear remains.

Though Lewis never offers a metaphysics of risk, the most plausible way to read him is that facts about probabilities establish the truth about risk, while elements of familiarity tempt people toward error. Alternatively, it might be the case that the word *risk* just means unfamiliarity, that calling something risky is a rough synonym for calling it unknown. If the latter is the case, people who are cautious about nuclear power may simply be good Burkean conservatives, people who resist new things, rather than people who have misjudged the low-probability/high-consequence facts about nuclear hazards. *See* my paper *Biotechnology, Risk, and Political Values: Philosophical Rhetoric and the Structure of Political Debate,* in BIOTECHNOLOGY: ASSESSING SOCIAL IMPACTS AND POLICY IMPLICATIONS 3-16 (D. Webber, ed. 1990).

[6]   Besides papers cited elsewhere, *see* Thompson, *Agricultural Biotechnology and the Rhetoric of Risk: Some Conceptual Issues,* 9 ENVT'L PROFESSIONAL 316 (1987); *Collective Action and the Analysis of Risk,* 2 PUBLIC AFFAIRS Q. 23 (1987); *Book Review of Rescher's Risk,* 9 ENVT'L ETHICS 91 (1987); *Risking or Being Willing,* 19 J. VALUE ENQUIRY 301 (1985); *Need and Safety: The Nuclear Power Debate,* 6 ENVT'L ETHICS 57 (1984); *Ethics and Probabilisitic Risk Assessment,* in BEYOND WHISTLEBLOWING: DEFINING ENGINEERING RESPONSIBILITIES 114 (V. Weil, ed. 1983); and *Book Review of Shrader-Frechette, Nuclear Power and Public Policy,* 30 CROSS CURRENTS 228 (1980).

but instead are rough substitutes for "correct," and "incorrect," or for "true," and "mistaken." It was in this vein that I interpreted Chauncey Starr as meaning to say that we are obligated to act upon the true facts, so far as we can tell what they are, when he discussed risk in terms of "the reality of what may or may not occur, the analysis of it, and our perception of it."[7] In my 1990 paper, I called this a *normative* use of the term "real risk," and sharply distinguished it from a methodological use in which the distinction between real and perceived risk is used (somewhat sloppily) to mark a distinction between the objects of research in natural and social science. Valverde misrepresents my view by running these remarks together, and by interpreting me to have been endorsing usage that I was merely describing.

It is the normative use of the expression "real risk," that is most important for law and policy, and which was the subject of my 1990 paper. Most people who use the expressions contrast real with perceived risk in a context in which it is clear that we should reject the risk estimates identified as "perceived," in favor of those characterized as "real." This is, for example, the intent of many scientific authors (such as Lewis[8]) whose analysis of statistical and experimental evidence convinces them that the public has an inordinate fear of nuclear power, pesticides and food additives. The common parlance is to say that the real risk is much lower than the perceived risk. The implication is that regulatory decisions should be based upon the scientific evidence.

While students of the social construction of risk will be able to supply many excellent reasons why this implication should be regarded with skepticism, there is something eminently common-sensical about an injunction to act on the true facts, rather than upon ignorant, reactionary or otherwise uninformed opinion. This injunction does not entail anything very systematic or philosophically rigorous with respect

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[7] C. Starr, *Introductory Remarks* in SOCIETAL RISK ASSESSMENT: HOW SAFE IS SAFE ENOUGH? 4 (R. Schwing and W. Albers eds. 1980). *See supra* my 1990 paper for the full discussion at 3–6.

[8] LEWIS, *supra* note 5.

to our concepts of "true," "fact," or "opinion." Indeed, its common sense value relies upon the ways that ordinary speakers of English use the words "true," "fact," and "opinion," rather than upon any careful philosophical theory of truth or of knowledge. Similarly, the usefulness of urging us to pay heed to the real risks (of pesticides, of nuclear power, of biotechnology), rather than perceived ones, relies upon fairly ordinary ways of using words like "real," "risk," and "perceived," rather than upon any rigorous analysis of these terms that might be given by philosophers or risk analysts.

Ordinary usage may or may not be supported by more rigorous conceptual or cognitive analysis of these terms. However, when rigorous theories substantially alter the patterns of ordinary use for such words, there is a danger of miscommunication and even deception. The danger materializes when authors use a word first in a common grammar, then draw an inference using the same word, but in a technical grammar. While we might be willing to accept the common sense of acting upon the true facts, so far as we know what they are, this does not commit us to accepting any philosopher's (or risk analyst's) theory of truth, facts, or risk as the basis for action. It was in this spirit that I offered "the causal answer," as an account of when risks are real.[9] We would be inclined to accept allegations of risk when they are supported by an account which identifies the unwanted event as the result of some cause or causes that are known, suspected or imagined to have occurred.

The claim that a risk is not real, but merely perceived, is, on this account, established by challenging the alleged link between cause and effect, or by explaining why the cause or causes have not or will not occur. My view is that our rigorous analyses of risk and causality are

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[9]   I will leave it to readers of this journal as to whether it is fair for Valverde to take me to task so severely for failing to offer a philosophically adequate account of causality. I will simply say that I am quite aware that my causal account only works so long as we *do not* have a philosophical theory of causes in science lurking in the background.

relevant to the normative use of the real/perceived risk distinction only when they are shown to be consistent with these non-technical, ordinary uses of the words "cause," and "risk." The non-technical concepts, in other words, become a test for technical analyses. While a technical semantics for these terms may sharpen or clarify the content of the non-technical claims, it must not commit the speaker of these words to a content substantially at odds with that of the non-technical usage. If the philosophical theory of "cause," or "risk," turns out to deviate from these non-technical uses, substitution of the technical concept into the normative injunction to base decisions on real (rather than perceived) risk is an equivocation.

As I interpret it, the causal answer to the question, "When are risks *real*?" does not entail any philosophical theories of scientific causality or rationality. Causation in the law is another matter. My goal was to examine situations in which ordinary people (e.g. people possessing no technical expertise) might ask such a question. The examples I produced in my 1990 paper describe situations (the possibility of a snake in the mailbox) in which someone is placed in the position of deciding whether to take an alleged risk seriously. More broadly, questions about the reality of risk are at least as likely to involve a judgment to hold someone morally or legally responsible for the consequences of an action as they are to involve *ex ante* assessment of probable outcomes. Although we routinely use the word "cause" when evaluating moral and legal responsibility, it is hardly clear that such usage will be supported by either deterministic or probabilistic theories of causality that are preferred in the sciences. In fact, some applications of such theories make it hard to hold persons accountable for the harms they cause, either because they make it appear that they could not have done otherwise (determinism), or they make it appear that the harm was just bad luck (probability) for which the agent cannot be held responsible. This latter problem is especially serious for Bayesians, as my 1990 paper was intended to show.

Valverde concludes by urging that, "Instead of focusing on probability numbers per se, probability should be viewed more as a general framework for structuring our reasoning about risks."[10] I should repeat that I find Valverde's reasons for rejecting a focus upon numbers compelling, but I would go much farther. I do not think that probability should be used as a framework for structuring our reasoning about risks. It is obvious that risk *involves* probability, but the rise of risk analysis has created widespread assumptions that the involvement of probability is more fundamental than it is. I have attacked this problem from a variety of angles in a series of papers,[11] and will not rehearse those analyses here. It is important to note merely that, in ordinary English, the word "risk," is an action verb. Risking is something that people (or other intentional agents) do. Probabilistic accounts of risk are idealizations which emphasize probability at the expense of the active component in ordinary grammar, but it is precisely the active component that allows us to hold agents morally responsible and legally liable for imposing risks upon others in a negligent, careless or imprudent fashion.

An alternative account would take the cognitive status of risk to involve a classificatory judgment. Acts consistent with a "reasonable man" standard are not thought to cause harmful effects suffered by others (at least not in the sense of "cause," relevant to holding the agent legally and morally responsible for harm). Harms that follow paradigmatically reasonable acts are "freak accidents," "acts of God," or "due to natural causes." While these categories will hardly do for a philosophical theory of cause and effect, they do quite well for identifying a broad class of actions that we may be confident of performing without elaborate or even explicit attention to the probability that harm to self or others will be the outcome. By contrast, acts classified as risky are subjected to additional burdens of proof before being judged acceptable. Formal probability considerations might enter

[10] Valverde, *supra*, at 337.
[11] Thompson, *supra* note 5.

in excusing an agent from responsibility for harm, but they need not enter in the judgment that determines an action to be risky in the first place. The cultural norms that determine what is risky in the sense of being exceptional, unknown or unreasonable are not fully consistent with the probabilistic correlation of harm to a given act. Many acts known by all reasonable people to have some probability of harm (boiling peas on the stove, driving to work) are nonetheless thought to be very ordinary, reasonable (e.g. not *risky*) acts.

It is, perhaps, ironic that paradigmatically reasonable acts both are and are not risks, they do and do not cause harm. It is simultaneously possible to talk about the risk of boiling peas, and to say that boiling peas is not a risky act. Such acts are risks (and do cause harm) in that they can clearly be correlated to harmful outcomes by standard statistical procedures. They are not risks (and do not cause harm) in that their very ordinariness exempts them from application of risk/benefit, consent, catastrophic loss and other criteria that are the standard burdens of proof for assessing new technologies.

I would agree with Valverde that risk analysis "... can be construed as a structured process that seeks to arrive at predictively informative assertions about possible events which, should they transpire, could pose a threat to human health and safety."[12] However, this statement goes a long way toward stipulating the specific policy context in which risk analysis is appropriate, namely one in which it is important to *predict* specific impacts upon human health and safety; but there are other legal, moral, and legislative contexts in which prediction is not the issue. A probabilisitic analysis of risk which gives us predictively informative assertions about possible events may well be (and I would argue, is) useless when applied to judgments about whether a drunk driver should be punished, whether a manufacturer should be held liable, and even whether securing consent from affected parties is mandatory. What I called the causal answer provides a better guide to

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12 Valverde, *supra*, at 323.

these cases, and any attempt to substitute a probabilistic framework will undercut established and effective principles for judging *ex post* responsibility for risk. Furthermore, the *ex ante* case of evaluating new technology may depend as much upon matters of intention, deception, lack of experience and consent as it does upon predictive information. As such, anchoring risk judgments in Valverde's probabilistic framework does not go nearly far enough in rejecting the reigning risk-analysis notion of real risk.

I will close by concentrating specifically on Valverde's comments on my criticisms of "risk subjectivism." My point was not to reject Bayesian concepts of belief revision *tout court*. I do not care whether Bayesian probability theory is an adequate account of scientific induction. I do not care whether it provides a good descriptive account of human cognitive processes.[13] My concern is that first replacing the broad categories of "the causal answer" with a probabilistic account of causes and then taking probability to be a function of a person's present information set establishes dysfunctional burdens of proof for addressing many risk issues. Bayesians commonly say things like Valverde's "... probabilities change or evolve over time; they are neither constant nor fixed." Although I am satisfied Bayesians do not (usually) mean it, they sound as if criteria for determining levels of risk are entirely relative to the information set that is available to a person (or organization) at the time that an estimate is made. If taken literally, such a view would license some novel defense strategies. Is a drunk driver's subjective estimate of the probability that she will have an accident the appropriate basis on which to base the judgment that she ran a punishable risk? Is a corporate engineering team's assessment of the

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13 Again, I must confess puzzlement at what Valverde attributes to me. He seems to say that I have misstated the Bayesian position by claiming that Bayesian updating somehow "invalidates" prior probability assignments. What I did say, at 21, was that, on a Bayesian view, new information "... cannot invalidate the judgment made on the prior, more limited information." I think we agree that Bayesian interpretations place the idea of "invalidating" prior assignments "at odds" with the concept of belief revision.

probability that O-rings sealing rocket engines will fail at freezing temperatures the one that counts in establishing moral responsibility or legal liability? Of course, when Valverde uses the word "subjective," he is merely categorizing the ontological status of probability under the mental or cognitive side of a metaphysical dualism. Let's see how *that* plays in a legal proceeding.

I am confident that there are interpretations of Bayesian probability that preclude such abuses, but I have yet to be convinced that they can be expressed as concisely and effectively as the causal answer. Once regulatory legislation has been enacted (and as long as it is unchallenged) agencies like EPA and FDA can use fairly technical approaches to the prediction of harmful events. Frankly, I doubt that it matters whether they adopt frequentist or Bayesian philosophies of probability, but if it does matter, that is not my present concern. The public debates in which the language of "real risk," surfaces are not well enough specified for technical concepts of risk to help. Either scientists will need to speak English (which means that "the causal answer," will do), or legislators, judges, juries, and maybe voters will have to learn to understand Bayesian conditionalization. The inadequacy of the latter alternative is, I hope, self-evident.